

RICHARDSON (Jos. G.)

ON

CERTAIN HUMAN PARASITIC FUNGI,

AND

THEIR RELATION TO DISEASE.

BY

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Read before the Biological and Microscopical Section of the Academy of
Natural Sciences, January 8, 1872, and ordered to be published.

24816

PHILADELPHIA:
J. B. LIPPINCOTT & CO.
1872.

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BY JOSEPH G. RICHARDSON, M.D.,

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A KNOWLEDGE that there do actually exist such unquestionable parasitic diseases as the potato-rot in the Vegetable kingdom, and the muscardine of silkworms in the Animal, has for many years led reflecting minds in the medical profession to reinvestigate, from time to time, ancient stocks of information, and to search after new facts in the relation of the lower to the higher organisms, of which we ourselves form so important an integral part, in order to discover whether analogues of the disorders named above are also to be found among the maladies to which humanity is subject.

Nor have these researches been by any means unfruitful; for although the observations of Hallier on cholera and typhoid fever, Salisbury on ague and small-pox, etc., are doubted or even ridiculed by a majority of the pathologists and microscopists in this country and Europe, on the other hand the experiments and deductions of Schoenlein, Gruby, Bennett, and Tilbury Fox, in regard to Favus and the whole group of parasitic cutaneous affections, are now fully accepted by most scientific physicians, and are contested by so small a number of medical men that, although this scanty minority is headed by the famous authority in dermatology, Erasmus Wilson, our doctrine of the existence of dermatophytic disorders in the human race may be considered to-day fairly established.

As I have elsewhere remarked, however, we see, at present, in regard to Favus and its congeners, the same old battle fought which fifty years ago was so strenuously contested by Bielt and Morgagni on the one hand, and the microscopists on the other, in relation to Scabies, now universally admitted to be due

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to a minute insect, the *Acarus scabiei* ; and for the student of human nature it is a most interesting confirmation of the wise king's dictum, "There is no new thing under the sun," to observe how the same doubts, followed by the same objections, were urged against the parasitic character of the itch, that are now put forward in opposition to the vegetable nature of Ringworm or Favus. Thus, Biett, Cazenave, Lugol, and others denied that even with the aid of microscopes of high power any insect whatever could be discovered. When by a succession of lucky accidents so many observers blundered into seeing the *Acarus* that this position was no longer tenable, opponents of the parasitic theory changed their base of operations, and, admitting the occasional existence of the insect, stoutly maintained that instead of being the *cause* of Scabies it was a secretory product of that affection, and by no means a constant one. Finally, when the *proof* became incontestable that the whole disease could arise from the deposit of a single itch-insect upon the skin of a previously healthy person, the anti-microscopists took refuge for years in the assertion that in such cases "the insect taken from the scabious vesicle, *being charged with the virus,—the fluid of the vesicle,*—by penetration of the cuticle inserted this virus, and produced the disease, like the inoculation of cowpox:" certainly a most plausible and ingenious invention, and one which, however improbable observed facts might render it, was nevertheless from the nature of things extremely difficult to disprove absolutely.

As an admirable example of the vegetable human parasites, I have the good fortune of being able to present for your inspection this evening a beautiful specimen of the *Microsporon furfur*, the fungus of the cutaneous disease known as *Tinea versicolor*, or *Chloasma*. It shows in a very distinct manner the groups of spores clustered together like bunches of grapes, and also the mycelial threads of the growth, which ramify among the epithelial scales of the skin where they flourished.

The case from which this was derived was that of a Belgian named August K—, about forty years old, who was admitted into the Pennsylvania Hospital on the 29th day of June, 1871. He had been for some time a sufferer from syphilis, and at the date of entrance into

the hospital exhibited deep cicatrices of rupia upon his legs. The eruption of *Tinea* occupied almost the whole anterior surface of the chest and upper part of the abdomen, but yielded readily to the application of an alkaline wash after its true nature was discovered. In this instance the complaint assumed, in its macroscopic as well as its microscopic characters, almost a typical form, since Dr. McCall Anderson, one of the most recent English authorities upon the subject, in his excellent manual of skin diseases observes that *Tinea versicolor* generally makes its appearance in little spots about the size of a pin's head, which gradually increase in size, retaining their circular form, but not healing in the centre, as is the case with ringworm. Gradually these spots unite, forming upon the trunk large irregular patches, near the edges of which we may usually detect small spots of eruption, which are very characteristic. The skin of the affected surface is scarcely if at all elevated above the surrounding parts, but is less smooth than in health, and is frequently the seat of a very fine desquamation, so fine as to resemble dust, and having a slight yellowish tint. The itching is generally moderate, sometimes almost absent, and occasionally very severe, although not specially aggravated at night.

The method of demonstrating the parasitic growth in this instance, when it was first detected by Dr. G. S. Gerhard, Resident Physician to the Pennsylvania Hospital, is quite simple,—a few fragments of the light-yellowish scales being merely scraped off the anterior surface of the man's thorax with a sharp scalpel, and examined in liquor potassæ, or, as was done with this particular specimen for convenience in mounting, in the saturated solution of the acetate of potash. In some other forms of dermatophytic disease, however, the recognition of a fungus is no easy problem, and the most careful investigation is necessary to insure success.

Dr. Tilbury Fox, in the last American edition of his work on Skin Diseases, New York, 1871, remarks:

“Many persons find the discovery of fungi in parasitic disease a difficult matter. The reasons for their non-detection are mainly as follow: 1st, from having too large a mass under examination. Thin sections or layers of epithelium or hair should be taken. 2d, the non-use of reagents to render the suspected tissues more or less transparent. 3d, too much manipulation. In this way spores are sometimes rolled up as

it were in epithelial layers, or softened and altered by reagents and thus concealed. 4th, the presence of pigment in large quantity. 5th, ill selection of hairs and scales. We may possibly extract for examination a healthy hair which stands in the midst of diseased ones; diseased hairs are loosened in the follicle, and altered in texture, dry and brittle. 6th, the fungus may be left behind in the follicle, the hair coming away without it. 7th, secondary changes are often mistaken for the real disease; scales may result from the irritation of a fungus not in them, but in parts near, and its absence from the same scales is no sign that the parasite is not the indirect cause of the scaliness. 8th, the stromal minute form is mostly overlooked."

As before mentioned, the patient from whom was taken the fungus I have now beneath the microscope upon the table, had a syphilitic history, and only by means of microscopic examination were we able to determine whether this eruption was specific in its character. Similar instances appear to be not unusual, for Dr. Fox observes, "I am constantly in the habit of seeing patients with Chloasma (*Tinea versicolor*) who have been treated vigorously for secondary syphilis. Such a mistake ought not to occur; it is generally when the chloasma is extensive that the error is made, but syphilis never produces a wide-spread fawn-colored staining like chloasma, and is never elevated and desquamating. The grand point of diagnosis between the two affections is the presence of the fungus in *Tinea versicolor*, and its absence in syphilitic disease."

As to treatment of this affection, the same author says, "I have one mode, and it is always successful. I apply a weak alkaline solution first of all, or wash with yellow soap, then sponge with a little weak vinegar and apply freely a lotion composed of four drachms of hyposulphite of soda and six ounces of water. A hyposulphite bath once or twice, if the case be obstinate, will aid somewhat; but I never require much else than this for any case."

Passing now to a more general consideration of the human parasitic fungous diseases, we are met at the threshold of our inquiry by the discussion of a very important and much-mooted point in phyto-pathology, analogous to that I have already mentioned as forming some twenty years since an epoch in the history of Scabies: to wit, whether the vegetable organisms re-

vealed by microscopic examination are primary causes of the inflammation, the itching, the loss of hair, etc., which they accompany, or whether these lesions are the results of some constitutional malady, which gives rise *also* to such local pathological changes in the tissues as permit the accidental development of the parasitic organisms. A few moments' reflection will suffice to convince us that this question is one of exceeding difficulty, and in fact I think it must be admitted that we have as yet been able to establish only a very strong probability in favor of the former of these views. Among the most conclusive arguments in support of such a doctrine—to wit, that parasitic fungi are the causes and not merely the accidental concomitants of the disease in which they are found—may be mentioned: *First*, their eminently contagious nature, as proved by the experiments of Dr. Hughes Bennett, wherein the spores and mycelial threads of the *Acherion Schoenleinii* transferred from the head of a patient affected with Favus, produced that disease upon his own arm and upon those of several gentlemen among his students. *Second*, the fact that these maladies are not merely contagious, but also infectious, and may be conveyed from one person to another by means of the spores which float in the air for a distance of at least several feet. *Third*, the prompt subsidence of the malady when complete destruction of the fungous growth by means of parasitocides is effected; and last, but not least, the remarkable experiment of Dr. Tilbury Fox, in which he transplanted a hair which was plucked from a patient affected with ringworm, and contained a few fungus-spores, to the rind of a lemon, which furnished sufficient moisture to keep it alive and allow it in its turn to nourish the parasitic *Tricophyton* that it contained, until splitting up of the hair was effected,—thus actually producing the *characteristic lesion of Ringworm out of the body*.

Dr. Tilbury Fox further remarks that—

“There is no difficulty in accounting for the access of germs to living bodies, for these germs are freely distributed and disseminated through the air. The best illustration of this fact may be noted in the experiments of M. Borzin (*Gazette Méd. de Paris*, July 30, 1864), which consisted in passing currents of air over the head of a favus patient, and thence over the open mouth of a jar containing ice. The ice cooled the air, causing the deposition of moisture, in the drops of

which the acherion sporules were detected. The same thing may be shown by holding a moistened glass slip near the head of a patient, and just rubbing his scalp freely. Of course, actual contact is much more effectual in the implantation of germs. Let us suppose that the sporular elements find their way to the human surface; how do they get beneath the tissues? Various ways, probably. The fungus-elements may enter by fissures or natural orifices; for example, in ordinary ringworm the sporules lodge themselves at the opening of the hair-follicles, and presently get beneath the epithelial scales. The growing mycelial thread forces itself beneath the layers of the superficial tissues, processes may shoot out from the spores themselves and enter beneath the epithelium, the spores may be enveloped and carried bodily inwards, or enter by traumatic lesions, as is said to occur in Madura foot. In each and every instance the germs of parasites are derived *ab externo*, and not generated *spontaneously*.

"There is yet one category of facts that needs a word or two of comment,—viz.: the comparative pathology or the inter-transmission of parasitic (vegetable) maladies. It is now admitted that the transmission of the common ringworm of the surface, from animals to man, is very common. I am informed upon good authority that this is of very frequent occurrence in Australia, the milkers of cows, especially, being largely affected. Professor Gerlach has noticed it in dogs, horses, and oxen, and in man, but the sheep and pig seem to offer exceptions. Dr. Frazer (*Dub. Quart. Journ. of Med. Science*, May, 1865) contributed a paper,—‘Remarks on a Common Herpetic Epizootic Affection, and on its Alleged Frequent Transmission to the Human Subject,’—containing cases. This gentleman quotes Mr. Brady and Mr. Whitla in reference to other instances. Dr. Fehr has noticed in Switzerland the transmission from cattle to man. I can confirm by my own experience the truth of these statements. Mice and cats affect man; mice with favus can communicate the disease to the cat, and the cat may give favus, or even tinea circinata subsequently, to the human subject."

Dr. R. Cresson Stiles reports in the *New York Medical Record*, vol. ii. p. 340 (1868), some interesting examples of Tinea observed in mice, from which the disease could readily infect cats and through them our own species, as probably took place in a case of Ringworm occurring some years since in my own practice.

But if, as is now generally conceded, fungous growths similar to that I have just shown you do flourish and produce disease whilst ramifying through and between

the epithelial cells of the skin and of the mucous membranes, as in Aphthæ, what valid reasons can be *a priori* urged against a belief that they may also develop among the cellular elements of the connective and muscular tissues; or even, if we adopt Prof. Virchow's definition that the blood is "a tissue of cells with a liquid inter-cellular substance," why may they not live and grow more or less luxuriantly in the liquor sanguinis, especially as it slowly permeates the capillaries and smaller veins?

That such is actually the case I think we have a large amount of evidence to prove; and one of my objects in the present paper is to lay before you some of the accumulated testimony upon this point, leaving, with one exception, any deduction from the fact—if fact it is accepted to be—for future consideration.

Prof. Lionel S. Beale, whose antagonism to the so-called germ-theory of disease is so well known that few will decline to admit as literally true any statements he may make sustaining the views its advocates hold, observes on p. 66 of his late work entitled "Disease Germs: their Supposed Nature" (London, 1870), "In many very different forms of disease these germs of Bacteria and probably of many fungi are to be discovered in the fluids of the body, but the evidence yet adduced does not establish any connection between the germs and the morbid process. In Plate IV. these minute organisms are represented in the contents of the alimentary canal and in the interior of the epithelial cells of the mucous membrane of the intestine in cholera. In the contents of the blood-vessels of the same disease, and in the blood taken from the vessels almost immediately after death of animals destroyed by cattle-plague and other fevers, similar bodies have been found, though probably not of exactly the same kind in every case. . . . Sometimes these germs grow and multiply in a secretion not perfectly healthy before it has left the gland-follicles, and they have been detected in the milk as it issued from the breast, in the saliva, in the bile and urine, as well as in other secretions. It will no doubt be said in all these cases, 'the germs have been introduced from without,—they pass from the air into the orifice of the duct, and thus make their way to the gland. From this point they might readily pass into the blood.' But it is more likely they are in the blood and in the

tissues at all times. They are met with in the blood especially in some instances in which there is no reason whatever for concluding they made their way into this fluid shortly before they were found. Nay, little particles may be seen in the circulating fluid which I believe to be these lowly germs, ready to grow and multiply whenever the conditions become favorable. I have seen such particles adhering to the surface of the white blood-corpuscles, and also to the red blood-corpuscles."

Prof. H. C. Wood, Jr., of this city, another able opponent of the germ-theory of disease, in his excellent article "On the Production of General Diseases by Organic Entities," remarks, "Whether the views of Prof. Wyman [regarding putrefaction] be correct or not, it is certain that under ordinary circumstances vibrios, etc. are always the accompaniments of putrefaction, and may frequently be found in a fluid before it has undergone more manifest changes. In the *milzbrand* of domestic animals and its probable derivative in man, *malignant pustule*, these bodies have frequently been found in the blood, but are said not to be present in all cases, and when present to be of fatal significance. *A priori* reasoning suggests that their spores are of very necessity at all times present in the blood; and the experiments of Frau Lüders seem to establish this. That lady took a small glass tube with the ends hermetically closed, which had been exposed for half an hour to a temperature of 290° C., and thrust it into the heart of a recently-killed guinea-pig and then broke off the ends. After the blood had been sucked into the tube from the other end, which was melted off to remove any fluid that might adhere from the lips, the ends of the tube were sealed, and it was kept at a temperature of from 13° to 15° C. After two days, fungous granules, chains, and rods were abundant. If then the spores of the vibronidæ be always present in the blood, they must of very necessity develop themselves whenever that fluid is strongly disposed to putrefactive changes, as it is in malignant pustule." (See *American Journal of the Medical Sciences*, October, 1868, p. 349.)

That some of the minute particles constantly found in normal human blood and partly constituting the globulins of Donné, the molecular substance of Griffith and Henfry, the microzymes of Dr. Burdon Sanderson and sundry French authors, and the germinal matter

of Beale,—at least in his earlier writings,—present the aspect of fungous spores, and develop into Bacteria-like bodies, is shown by the following experiment, which I have repeated a sufficient number of times to convince myself that I have not been misled by any false interpretations of the appearances presented.

A drop of blood from the finger, drawn with careful precautions against adulteration, was covered with a large thin glass in such a manner as to include a few bubbles of air, and the whole hermetically sealed by a layer of gold-leaf applied at the margin of the covering glass. A suitable field of view being selected, containing between the margins of red corpuscles several large open spaces, in which could be seen with a $\frac{1}{2}$ -inch objective a number of the tiny particles above referred to, each in active movement—perhaps molecular in its character—the slide was firmly secured in position, and careful observations and drawings were made at short intervals, except through the night, for three days, at the end of which period the minute granules first seen had developed to three times their original size, becoming about $\frac{1}{100}$ of an inch long, assuming an elongated dumb-bell shape, and increasing in number so that the place of a primary particle would often be occupied by six, eight, or ten Bacteria-like bodies, sometimes irregularly grouped together, but quite frequently assuming the beehive-like arrangement so characteristic of the central mycelia of the lower vegetable organisms.

A series of my own experiments performed to demonstrate the presence, mode of entrance, and pathological effects of bacteria in human blood, were detailed in the *American Journal of the Medical Sciences* for July, 1868, from which I extract the following as the most important:

“Experiment 4.—At 7.45 P.M., May 17, 1868, I drank four fluidounces of water similar to that employed in the preceding investigations, and containing multitudes of bacteria (estimated as numbering 27,000,000,000). At a quarter past eight I examined a drop of blood drawn with a sterilized needle from the tip of my finger, and confined between a slide and cover-glass with strong hydrochloric acid as above described. Under the field of the one-twenty-inch inch glass the interspaces between the rows of blood corpuscles were found to contain multitudes of apparently spherical molecules, in rapid and constant motion,—but so very minute as to readily escape notice even with this high power, except under the

closest scrutiny; in the course of half an hour not less than one hundred were observed. At 9 P.M. another drop of blood, examined with the same precautions, exhibited, in addition to these minute particles, other bodies, less active in their movements, of much greater magnitude, and which under an amplification of eleven hundred diameters appeared precisely similar to the bacteria I had been studying a few hours before in the identical decomposing beet juice imbibed.* Five of them were thus enlarged sufficiently to exhibit an unmistakable organized structure totally different from their associated aggregations of Beale's germinal matter. Three of these bacteria were each about $\frac{1}{100,000}$ of an inch in length and $\frac{1}{100,000}$ of an inch in width, very distinctly constricted in the middle; a fourth was obviously composed of four, and a fifth of six joints arranged in a straight line, whose motion was of that peculiar waving character so universal among the Oscillatoriaceæ. The last two were most clearly visible when they happened to lie vertically to the surface of the glass, and would probably escape observation under the one-eighth-inch except in that position, or be therefore mistaken for simple globular bodies, although in several cases I detected in the second and third experiments (with a lower power) a shadowy elongation of one diameter on the revolving molecules thus observed."

These results which I obtained were not confirmed during a repetition of my experiments made partly under the supervision of Col. J. J. Woodward, of the Army Medical Museum at Washington, but which were not, as has been stated by him, uniformly without any effect; and although such evidence must of course be allowed to have for negative testimony unusual weight, it should, I think, be viewed in conjunction with the observations of Dr. Neffel, of New York City, and M. E. Semmer, of Dorpat, whose researches go far to confirm my conclusions.

Dr. Neffel remarks (see *Medical Record*, July 15, 1868, p. 226), "Finally, I may mention the experiments which I myself made last year in Prof. Virchow's Pathological Institute and Dr. Kühne's Chemical Laboratory, and which I still continue. My object was to study the influence and mode of action of cryptogams in the animal body. For this purpose I produced a fistula of the small intestines in dogs, and through it introduced

* Careful and repeated observations had of course been made to establish the fact that similar particles were not visible in my blood immediately before drinking the bacteria.

the fungi. I also injected cryptogams into the respiratory organs of dogs and rabbits by means of tracheotomy; again I injected them hypodermically; into the veins; into the abdominal cavity; and at last I introduced them into the lymphatic sacs of frogs and observed by a obituary's method!" and after giving a detailed account of particular investigations, he observes, "My experiments so far lead me to the conclusion that the lower vegetable organisms can continue to live and multiply in the tissues of living animals, and that they can enter into the general circulation either through the intestinal canal or respiratory organs or by means of hypodermic injections. What is their ultimate fate in the animal organism, and what their importance in producing disease, further investigations will have to show."

In further corroboration of my researches I may quote the experiments of M. E. Sommer, Professor to the Veterinary Institute of Dorpat, described in his paper on the "Results of Injection of Fungous Spores and Fungous Cells into the Blood of Animals." This gentleman states that whilst in the blood of creatures affected with glanders, splenic inflammation, and septicæmia, micrococci cells, mycelia, and hyphæ chains and threads are common, the circulating fluid of healthy beasts contains generally a few similar cells, and anadogonæ rods and chains are to be met with in the liver and intestine. He aims details three series of experiments made to test the question whether these contagious diseases were caused by the fungi found in the blood, and fully narrates the post mortem examination of a foal into whose jugular vein was injected about two ounces of distilled water containing countless fungus spores and micrococci cells, cultivated from splenic disease (*asplenicæ*) blood upon heated material, the poisonous dose causing the death of the animal on the eleventh day, preceded by loss of appetite, tottering gait, and marked febrile action. (See *Proben's Archiv*, Heftrigsten Band, erster Heft, S. 159, Berlin, 16. April, 1879.)

Influenced, therefore, by the positive statements of Drs. Daniel S. Beale and H. C. Wood, Jr., and the direct observations of M. Davaine, Dr. Netzel, and M. E. Sommer, as well as by the results of my own personal investigations, I think we must admit that Bacteria not only live but flourish in the blood of animals and of man with more or less frequency during the course of

various maladies; yet whether by so existing in the circulating fluid these vegetable organisms constitute causes of disease, or whether they are simply products, or, again, mere accidental accompaniments of morbid action, it appears to me we have not hitherto accumulated a sufficient number of *facts* to enable us to decide.

Hence I think the profession is as yet unprepared to accept the doctrine of Dr. A. E. Sansom in his late work, "The Antiseptic System," London, 1871, p. x., where he says, "From a review of all the facts and observations I have been led to enunciate the theory that the poisons of spreading diseases are extremely minute living organisms, having the characteristic endowments of vegetable growths analogous to the minute particles of vegetable protoplasms whose function it is to disintegrate and convert complex organic products, owing their specific properties in special diseases not to any botanical peculiarities, but to the characters implanted in them by the soil in which they first sprang from innocuous parents, and from which they are transmitted, —this soil, except in the case of their earliest origin, being the fluids of the animal body."

In order, however, that such researches as those above referred to may be made, even in their present incomplete state, to aid us in improving the science of medicine, permit me to suggest that each Bacterium existing in the blood (whether as a poisonous cause, a product, or an accident of disease, I refrain from discussing at present) must during every moment of its life appropriate some minute portion of pabulum, which would otherwise have contributed to nourish the tissues of the animal in which it resides; and that therefore the sum total of the Bacterian influences (unless they feed solely upon effete matters, unlike their analogues of the dermatophytic affections) *must* be effective towards diminishing the vital power of the organism on whose life-blood they prey.

This view is confirmed by the experiments of Dr. Burdon Sanderson in his late interesting memoir "On the Origin and Distribution of Microzymes [Bacteria] in Water, and the Circumstances which determine their Existence in the Tissues and Liquids of the Living Body," in which he says, "As regards their [the Microzymes'] action on the liquids in which they live, the most important facts are (1) that their growth is attended with

absorption of oxygen and discharge of carbonic acid ; (2. that they are remarkably independent of the chemical constitution of the medium, provided that they are supplied with oxygen ; and (3. that they take nitrogen from almost any source which contains it, and use it for building up their own protoplasm.) It is this last power which specially indicates what may be called their place in nature, as the universal *deobrevers* of nitrogenous substances, acting as the pioneers, if not the producers, of putrefaction. They exercise this function, not by virtue of any special relation of their own nutritive processes to putrefaction as such, but simply by their extraordinary power of acting on the elements which they require for the construction of their own bodies. (*See Quarterly Journal of Microscopical Science*, October, 1871, p. 326.)

Upon this doctrine it is not difficult to found a theory for explaining the probable mode of operation of quinine and arsenic,—so long a problem in therapeutics,—when acting as tonics upon the human system in many cases of disease: namely, that in part, at least, they serve these important purposes by rendering the blood fit for the development of these lower organisms, and in this way *renewing* the supply of nutritive material in the circulating fluid. Many well-known preparations in the action of these two principal tonic medicines tend to confirm such a belief,—as, for example, the facts: *First*, that quinine and arsenic, two substances which, although unlike in almost every other respect, resemble each other, as shown by the experiments of Dr. Beaumont and Dr. Dougall of Glasgow, in being powerfully inimical to vegetable life, are in many cases our most reliable tonics. *Second*, that as a general rule, three or four days must elapse before a decided invigorating effect is produced upon the system by these remedies,—a period which, we may conclude, is requisite for sufficiently impregnating the blood (without disturbing the digestive organs) to render it an unfavorable medium for the growth of Bacteria. *Third*, that after a continuance of some weeks these roborants generally lose their invigorating power upon any particular patient, but seem to have regained it if recommenced after their administration has been for a short time interrupted,—*i.e.* subsequent to the development of a new crop of Bacteria, which they again

destroy. And *Fourth*, that arsenious acid has so remarkable an effect upon the arsenic-eaters of Styria, producing such fat, vigorous, and pure-complexioned individuals when steadily continued, and giving rise to such intense suffering if omitted for a short time ; results which can hardly be owing to the arsenical preparation supplying any necessary constituent of the human body, and which seem much more probably to be due to some action in preventing a waste of nutriment, and consequently of vital power.

BIOLOGICAL AND MICROSCOPICAL SECTION OF THE ACADEMY OF NATURAL SCIENCES.

MONDAY, JANUARY 8, 1872.

DIRECTOR W. S. W. RUSCHENBERGER, M.D., in the chair.

Present also—Prof. Jos. Carson, Prof. R. E. Rogers, and Messrs. Tyson, F. W. Lewis, Nancrede, I. Norris, Schaeffer, McQuillen, Holt, Buckingham, Corlies, West, Packard, Wood, and Richardson.

Visitors—Commodore J. P. Gillis (M.A.N.S.), Dr. Tomey, U.S.N., Dr. L. A. Duhring.

Dr. JOS. G. RICHARDSON read a communication on "Certain Human Parasitic Fungi and their Relations to Disease," illustrated by specimens of the *Microsporion furfur* mounted in a saturated solution of acetate of potash. In this paper, after a description of *Tinea versicolor*, the author maintained the view that other tissues besides the skin might be the seat of parasitic growths, as indeed had been shown was the case in regard to the blood, in which, according to the observations and experiments of Beale, H. C. Wood, Jr., Dayaine, Neffel, Semmer, and the writer, Bacteria may flourish ; and on this fact Dr. Richardson founded a theory of the tonic effect of quinine and arsenic,—to wit, that these remedies acted by preventing or retarding the growth of Bacteria in the circulating fluid.

Dr. TYSON remarked that minute particles of germinal matter in the vegetable and animal kingdoms are often so entirely identical in their morphological characters that they cannot be distinguished from each other ; so that the spores

described by these various investigators as seen in the blood may have been the earliest forms of animal, and not of vegetable, organisms. He was himself inclined to doubt that the sporules Dr. Richardson found in his blood after drinking water filled with Bacteria were the same as those occurring in the fluid imbibed, for the reason already stated, and because the gastric juice has, as is well known, a powerful destructive action upon all bodies consisting of or containing albumen.

Dr. RICHARDSON observed that the existence and growth of *Sarcina ventriculi* in the stomach showed that the gastric fluid was not necessarily destructive to vegetable life, and mentioned that the strongest argument that Bacteria such as he drank are non-albuminous, vegetable, and *not animal* in their nature, is found in the fact that they are unacted upon by a solution of caustic potash.

Dr. TYSON answered that the *Sarcina* developed during the occurrence of pathological conditions of the stomach which there was no reason to suppose existed in Dr. R.'s own case when he made the experiment referred to.

Dr. RICHARDSON replied that he believed it was stated by Dr. Beale that the flocculi containing *Sarcina* in vomited matter were always intensely acid, and, although he knew of no analysis of the fluid, it was fair to presume that at least part of this acidity was due to the gastric juice.

Prof. CARSON remarked that, if we accept this theory of the tonic action of quinine and arsenic, iron, gentian, and the whole catalogue of roborants must receive the same explanation of their effects. From his own experience, he had been led to believe that arsenic produced a desire for food by its irritant action upon the coats of the stomach; and also that its tonic and anti-spasmodic power had been very much overrated, especially as its effect upon the Styrian peasants was after a time to produce a dropsical condition of the whole body. He would like to inquire whether a fungus would not grow in solutions of quinia and of arsenic.

Dr. RICHARDSON replied that such was the case with certain kinds of fungi, but that others, and especially the Bacteria or Microzymes in question, had been shown to be killed by even very dilute solutions of these two agents.

Prof. CARSON further remarked that quinine had a powerful influence upon the nervous system, and that its effect upon the organism in general was certainly made up of several different elements. Recurring to Dr. R.'s experiment with Bacteria, it was difficult, he thought, to account for the entrance of these bodies into the capillaries, whose walls only after much dilatation would give exit to the Leucocytes of the blood, as is seen in Cohnheim's experiment.

Dr. H. C. WOOD, JR., observed that during the past few years he had enjoyed opportunities of observing intermittent fever very extensively in the Philadelphia and Episcopal Hospitals, and had experimented upon a great number of cases with the sulphites and with carbolic acid. The conclusions at which he had arrived were, that the latter of these so-called remedies had no more effect in checking the disease than so much cold water, and that the former were almost but not quite equally inert. He had, however, found arsenic to relieve very obstinate cases of ague with great certainty.

Dr. TYSON said that in some experiments of his own, conducted with this special object in view, he had found that the spores and threads of a fungus developed in a saturated solution of sulphite of soda were uninjured when macerated in the fluid after the liberation of sulphurous acid from the sulphite by the addition of an acid.

Prof. ROGERS remarked that this subject had an important bearing from a chemical point of view, on account of its close association with some of the phenomena of fermentation. It has long been known that the addition of sulphites or hyposulphites will obviate the loss of sugar by fermentation during the clarifying process, and also that the same compounds of sulphurous acid will prevent the conversion of barrelled cider into vinegar. The explanation is that the sulphites are converted into sulphates by the absorption of free oxygen, which absorption prevents the development of the vegetable organisms on whose growth fermentation depends, since oxygen is absolutely necessary to the germination of such fungous spores.

Prof. CARSON observed that some forty years since he was acquainted with a bottler who was accustomed to preserve cider by covering it with a layer of sweet oil, this doubtless having the same effect of cutting off a supply of oxygen.

Dr. RICHARDSON suggested, in regard to the tonic effect of iron in its various preparations, that it might be accounted for on the supposition that it acted by supplying a necessary ingredient to the red blood-corpuscles, and so increasing their number, when deficient, as in anæmia.

Prof. CARSON approved of this explanation, and added that there was also another way in which ferruginous medicines acted powerfully in improving the tone of the system, and that was as restoratives of the blood-making function, and nutritive alterants. He had known many cases of ague where quinine failed to accomplish a cure, until after the patient had been subjected to the depurative action of tincture of chloride of iron with sweet spirit of nitre, through the kidneys.

